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AMPÈRE'S ELECTRODYNAMICS AND THE ACCEPTABILITY OF GUIDING ASSUMPTIONS

1. INTRODUCTION

The primary target of the following essay is an important historical thesis (GA 1.3) promoted in various forms by Imre Lakatos¹ and, subsequently, by Elie Zahar and John Worrall.² A closely related claim also to be scrutinized, is (GA1.4).

In order to test (GA1.3) in the case of Ampère's electrodynamics, I begin by stipulating a meaning for the term 'novel prediction'. This concept has generated a considerable variety of interpretations due to conflicting opinions about the most relevant way to apply it in the analysis of actual scientific debate. Ampère's attempts to promote acceptance of his program in electrodynamics provide a clear test for the historical significance of the type of novelty Michael Gardner has labelled 'use novelty'.³ My conclusions thus bear most directly upon John Worrall's revisions of Lakatos's ideas, in that Worrall incorporated 'use novelty' into his concept of 'genuine empirical support'. My assessment of (GA1.3) thus addresses Worrall's rendition of this thesis in terms of 'use novelty' more directly than any of Lakatos's numerous earlier formulations.

With this understanding of 'novel prediction' at stake, I present evidence that Ampère did call attention to the importance of novel predictions prior to the beginning of his research in electrodynamics in 1820. After a summary of the guiding assumptions of Ampère's electrodynamics, I test (GA1.3) by an analysis of the arguments Ampère offered for the acceptability of his program. To avoid a potentially misleading reliance upon a single episode, I assess Ampère's prolonged apologetics during the formative and highly competitive period between 1820 and 1823.

My general conclusion is that, when 'novel prediction' is understood in terms of 'use novelty', Ampère's argumentation does not rely upon a significantly clear-cut stipulation of this concept. His language is sometimes ambiguous or general enough to include the criterion cited in (GA1.3), but we certainly cannot conclude that he wanted his program

to be judged largely according to this criterion. It is clear that Ampère relied heavily upon other types of evidence, one of which is cited in (GA1.4). In this respect (GA1.3) is not confirmed by Ampère's argumentation.

The present essay is almost entirely limited to the criteria operative in Ampère's own attempts to promote acceptance of his program. A more complete analysis would have to investigate a wide variety of scientists to determine what factors were responsible for the stance they took with respect to Ampère's program. In my concluding remarks I present some evidence that (GA1.3) is not substantiated by the criteria employed by Ampère's colleagues.

2. NOVEL PREDICTIONS

The starting point for Imre Lakatos's analysis of scientific change is the recognition that sequences of closely related scientific theories evolve through an application of the much more general sets of guiding assumptions he called research programs. According to Lakatos, these assumptions include both a "hard-core" of primarily ontological or metaphysical commitments and a 'positive heuristic' intended to guide the creative process of theory revision within a program. A scientific revolution involves a widespread shift in allegiance from one research program to another; the assessment of the guiding assumptions promoted by rival research programs thus becomes a central issue in Lakatos's analysis of scientific debate.

In particular, Lakatos insisted that scientists expect a well-founded research program to generate theories that are not simply created by ad hoc adjustments of earlier theories due to confrontation with anomalous phenomena. That is, scientists are claimed to prefer 'progressive' research programs in which theories should not only account for the anomalies that plagued their predecessors, but should also successfully 'predict novel facts'. Lakatos originally formulated this criterion in terms of strictly temporal predictions of hitherto unobserved phenomena.⁴ Realizing that this requirement was too strict to reflect accurately most actual scientific practice, Lakatos repeatedly revised his concept of 'novel fact' before finally agreeing to a definition proposed by Elie Zahar.⁵ According to Zahar⁶

A fact will be considered novel with respect to a given hypothesis if it did not belong to the problem-situation which governed the construction of the hypothesis.

Unfortunately, as Michael Gardner has argued,⁷ it is difficult to see how this rendering of novel fact could function very readily in scientific debate. The potential domain of applicability originally envisioned for a theory may perhaps be kept private by its originator and, thus, not be available to other scientists attempting to judge the merits of the research program concerned.

John Worrall's response to this problem was to reconceive novelty in the sense subsequently labelled 'use novelty' by Gardner.⁸ Following Lakatos's terminology, Worrall still holds that one criterion for the comparison of rival research programs involves determining which program has been most "progressive". But for Worrall a program is relatively progressive insofar as its theories progress beyond their predecessors by being 'genuinely supported' by more empirical facts. That is, a theory and its research program are only genuinely supported by an empirical fact when the theory entails a description of the fact, and when that fact did not contribute to the production of the theory through a modification of its predecessor within the program; instead, the theory must be generated from its predecessor by an application of the positive heuristic of the program without relying upon the information provided by the fact in question. On the other hand, the mere explanation of a fact through a *post hoc* modification of a prior theory in direct response to the fact to be explained does not provide genuine support for the theory so generated; the research program concerned is stigmatized as 'degenerate' if this tactic has become its sole means of increasing its empirical scope.

The importance of the guiding assumptions making up a positive heuristic leads Worrall to cite heuristic strength as a second criterion for the appraisal of research programs. The basic touchstone for heuristic strength is the degree to which the assumptions of a positive heuristic provide a clear-cut strategy for the creation of a progressive sequence of theory revisions, that is, a strategy that specifies techniques for theory revisions that result in an increase in genuinely supportive empirical scope. In physics, for example, a program is being guided by a strong positive heuristic when the 'hard-core' conceptual, dynamical and ontological assumptions of the program are expressed in a mathematical

structure appropriately complex as to suggest specific tactics for progressive theory revision in anticipation of the inevitable discovery of anomalous new experimental evidence.

The task of the present essay thus is complicated by the fact that, in contrast to the formulation used in theses such as (GA1.3) and (GA1.4), Worrall's version of the methodology of scientific research programs does not claim that any one of the criteria cited in these theses is to be singled out as the one that is 'largely' appealed to in scientific debate. While I shall argue that debates in early-19th-century electrodynamics did not rely predominantly upon the progressive criterion cited in (GA1.3), this does not necessarily impugn Worrall's less exclusive attribution of importance to both progressiveness and heuristic strength. But this argument requires a brief preliminary summary of the guiding assumptions of Ampère's program.

3. THE GUIDING ASSUMPTIONS OF AMPÈRE'S ELECTRODYNAMICS

Oersted's 1820 discovery that the orientation of a suspended bar magnet could be altered by an electric current initiated an intensely competitive debate over the new domain Ampère soon labelled 'electrodynamics'. By November 1820, Ampère had made the experimental discoveries and conceptual interpretations that inspired his adoption of the guiding assumptions for a new research program.⁹ These assumptions can be briefly summarized as follows.

Based upon Oersted's discovery and his own discovery of attractions and repulsions between current-bearing conductors, Ampère held that all electrodynamic phenomena, including all purely magnetic effects, are produced by forces acting between particles of two electric fluids moving within electric currents. Although the exact trajectories of these motions could not be specified, Ampère asserted that the most fundamental electrodynamic forces were central forces acting along the line joining pairs of electric fluid particles. He also believed that electrodynamic effects were propagated due to the transmission of a state of polarization within the electric fluids constituting an all-pervasive ether.

Secondly, Ampère claimed that all magnetic phenomena are produced by electrodynamic forces arising from electric currents confined to circuits within the neighborhoods of individual molecules of magnets.¹⁰

Although Ampère's most fundamental ontological assumptions about

electric fluids were not directly testable, these claims were supplemented by additional heuristic assumptions about how the observable effects of electric fluid particles were to be shown to be consequences of empirically testable laws pertaining to electric currents. These laws were to be formulated without reference to the electric fluid micro-structure of electric currents. That is, Ampère assumed that all electrodynamic phenomena could be given a preliminary explanation through the application of a law stating the force between electric circuit segments small enough to be considered 'infinitesimal', but only in comparison to measurable magnitudes. If this law could be determined, it was assumed that mathematical integrations of it would be applicable both to directly observable circuits and to the molecular circuitries of magnets.

The sequence of theory revisions generated by Ampère's program thus was expected to provide increasingly accurate magnetic circuitries and more detailed expressions for the electrodynamic force between circuit elements. In order for his program to be judged 'progressive' in Worrall's sense, these theories would have to achieve genuine empirical support from phenomena not used to invent them. Thus, when (GA1.3) is interpreted in terms of 'use novelty', the thesis implies that the acceptability of Ampère's guiding assumptions was assessed largely on the basis of the genuine empirical support garnered by the sequence of theory revisions produced by his program.

4. AMPÈRE AND NOVEL PREDICTIONS PRIOR TO 1820

Ampère's study of electrodynamics came at a relatively late stage in his unconventional career. By 1820 he had reached the age of forty-five without making any systematic contributions to physics. The majority of his professional activities were performed as a mathematician and were supplemented by erstwhile excursions into chemistry;¹¹ his primary interests were metaphysics, psychology and epistemology. Although Ampère never brought his voluminous notes on these subjects into a publishable condition, they nevertheless provide valuable insights into his methodological views prior to 1820.

In particular, it is clear that Ampère originally held the successful prediction of new phenomena to be the most important type of confirmatory evidence for a theory. For example, in some notes pertaining to his earliest speculations about electricity in 1801, Ampère claimed

that in proposing a *système physique* as a causal foundation for a given domain,¹²

... one has all the more means of verifying it as one has used fewer in discovering it. One has only to imagine some new experiments and predict their outcome; it then suffices to try them in order to see if the conjectures one has made are confirmed or not.

In this early passage, Ampère clearly has in mind a strictly temporal prediction pertaining only to phenomena hitherto unknown. Furthermore, he seems to restrict confirmatory evidence to these successful predictions just as was required by Lakatos's original definition of 'novel facts'.

By 1814, however, Ampère had liberalized his attitude toward confirmation so as to approach Worrall's conception of genuine empirical support. In that year he commented as follows concerning his hypothesis that all gases at a given temperature and pressure have the same inter-particle separation.¹³

Whatever may be the theoretical reasons which seem to me to support it, one can only consider it as a hypothesis. However, when comparing the consequences, which are the necessary implications of it, with the phenomena or the properties that we observe, if it is in agreement with all the known results of experiment, and if one deduces consequences from it that are found to be confirmed by later experiments, it will acquire a degree of probability which will approach what, in physics, is called *certitude*.

Although in this passage Ampère does not attempt to specify the relative weighting of the two types of confirmatory evidence he cites, he clearly continued to give predictions of new phenomena an important status.

In 1816 he made some additional methodological remarks in terminology strikingly comparable to Worrall's. According to Ampère, when a scientist is confronted by a choice between alternative theoretical explanations, he should¹⁴

... definitely adopt the one explication that seems the most probable only after having verified it by drawing some consequences from it which are a necessary implication of it and which are confirmed by some facts that one did not have recourse to during the research which led to it and which these consequences themselves indicate to the observer in the case where he would not yet have noticed them.

Although the qualification appended in the last phrase of this passage is not of optimum clarity, Ampère may very well have been trying to express a conception of genuine empirical support. He seems to be

putting his emphasis upon the fact that decisive evidence is provided by all entailed facts that were not used to create the theory in question; temporal prediction of new phenomena does not appear to be essential.

Prior to 1820, Ampère's methodological remarks thus provide some preliminary evidence that he was predisposed to judge the guiding assumptions of a research program according to how successfully its theories make novel predictions. But be this as it may, in order to address (GA1.3), we must investigate the extent to which this predilection was put into practice in actual debate in electrodynamics.

5. AMPÈRE'S ASSESSMENT OF CONFIRMATORY EVIDENCE DURING 1820 AND 1821

When (GA1.3) is interpreted in terms of 'use novelty', we should expect that, when Ampère began to argue for the acceptability of his guiding assumptions late in 1820, he would rely 'largely' upon the degree to which his theories achieved genuine empirical support from phenomena not used to construct them. In fact, however, we find that he also relied upon other factors and did not restrict himself to genuine empirical support as his primary mode of appraisal. Thesis (GA1.3) thus is not confirmed by the period prior to 1822.

First of all, in some of his earliest comments on the superiority of his program in contrast to Biot's rival magnetic fluid program, Ampère conceded that many phenomena could be explained in a non-quantitative manner by both programs; he made no effort to specify which of these phenomena had been used to invent the explanatory theories in question. Instead, he stressed that his own general assumptions offered the twofold advantage of ontological simplicity and conformity to the longstanding French penchant for central forces.¹⁵ Ampère thus was quite willing to promote his program on the basis of standards quite independent of empirically successful prediction.

A second reason for denying the truth of (GA1.3) during 1820 results from a scrutiny of how Ampère publicized the extensive set of new electrodynamic effects explained by his program during November and December. During this period Ampère did indeed successfully confirm some important predictions drawn from his assumption that magnetic effects are due to internal electric circuits. Most importantly, he duplicated many of the effects of bar magnets and magnetized needles using

electric circuits in the form of large suspended loops, tightly-wound spirals, and helices of small cross section.¹⁶ But many of these explanations were only made possible by an important new theoretical assertion Ampère proposed in order to explain what had originally been a seriously anomalous phenomenon.¹⁷ Nevertheless, whenever Ampère had occasion to tabulate favorable evidence, he made no effort to exclude this particular experimental fact just because he had used it to make an important theory revision. Ampère thus did not employ the 'use novelty' distinction in the manner (GA1.3) implies that he should have.

During the subsequent year of 1821, Ampère's argumentative strategy was influenced by two constraints. First, he still could not claim superiority over Biot's program either on the basis of genuine empirical support (assuming that he would have wanted to) or on the basis of a more inclusive conception of empirical confirmation. Although he repeatedly attempted to perform conclusive 'crucial experiments' to decide between his program and Biot's, most of the resulting observations turned out to be equally well explained by both programs. In fact, in January 1821 the one experiment that did not fall into this category turned out in favor of Biot's program and was tactfully ignored by Ampère for over two years until an important theory revision allowed him to explain it.¹⁸

Secondly, Ampère encountered serious calculational difficulties when he attempted to apply his force law to any circuits more complicated than two parallel wires.¹⁹ This meant that he could not upstage Biot by providing quantitative explanations for the phenomena they both had explained less rigorously.

Ampère summed up this situation in an expository memoir written in March 1821. After summarizing the known facts to be explained, Ampère pointed out how he had been able to derive an expression for his electrodynamic force law solely from a study of phenomena pertaining to experimentally-manipulated electric currents; he had not needed to rely upon phenomena produced by magnets and their alleged internal currents. Ampère's important concluding comment is worth quoting in full.²⁰

These results, based on necessary deductions from the data of experience, cannot be placed in doubt as long as they concern only the mutual action of conducting wires discovered by M. Ampère.; he extends them not only to what M. Oersted has made

known to us concerning a wire conductor and a magnet but to those actions of two magnets on each other in conformity, as we have said above, to the manner in which he conceives that electricity arranged in magnets produces all the magnetic phenomena. This extension given to the mathematical laws of the mutual action of voltaic conductors will decide the question of the identity of electricity and magnetism; this question is reduced by M. Ampère's work to a question which is solely within the province of mathematical analysis, since it is only a matter of calculating, according to the formulas which he has given, all the circumstances of the mutual action of two magnets which physicists have observed or measured up to the present and of seeing if the results of these calculations constantly accord with the data of experiment.

The first point to be noted in this passage is that Ampère does not make any explicit claim that magnetic effects and phenomena produced by interactions between magnets and electric currents are domains which provide particularly important evidence for the accuracy of the mathematical expression he had determined for his electrodynamic force law between current elements. He does not attempt to foster acceptance of his guiding assumptions about this force by appealing to the criteria cited in (GA1.3, GA1.4) and (T2.2).

Secondly, in his remarks about the quantitative application of his force law to the two domains where magnets are concerned, Ampère foresees a decisive confirmation of his assumption about the electric basis for magnetism. Nonetheless, contrary to (GA1.3), he does not foresee any need to isolate those facts used to stipulate magnetic circuitries from the more general set of all facts to be explained by a subsequent application of the force law to these circuitries. Ampère's comments are conspicuously void of any reference to the "use novelty" distinction.

During the remainder of 1821, Ampère's rhetoric shifted slightly in response to further unexpected developments. Inquisitive scientists throughout Europe continued to discover new electrodynamic phenomena which they sometimes cited as anomalous counterexamples to Ampère's program. Ampère typically responded to such discoveries and allegations by pointing out that, although he could not yet calculate the magnitude of the new effects, a non-quantitative study of the implications of his force law could at least account for their existence. Furthermore, these *post hoc* explanations were important confirmatory evidence because his latest theory "could have been used to predict them in advance".²¹ But although Ampère's remarks during this period do indicate that he did not restrict his concept of relevant evidence to

successful predictions of new phenomena prior to their experimental confirmation, he gave no indication that his program should be judged 'largely' upon the basis of 'use novelty'.

6. AMPÈRE AND NOVEL PREDICTION DURING 1822

Early in April 1822, Ampère concluded a discourse at the annual public session of the Académie with the following contrast between his program and Biot's:²²

It is in this way that, of two hypotheses serving to explain a certain number of phenomena, the one which can account for them only by striving to agree with them is ordinarily refuted by other phenomena for which time eventually brings the discovery; on the other hand, the one which is simply, so to speak, the expression of the true relations of the facts which it explains, is found confirmed each time experiment makes new ones known to us.

Ampère thus implied that Biot's program had degenerated because it had relied upon *ad hoc* tailoring of theory in response to anomalous phenomena. However, Ampère made no attempt to explain how, prior to the rotary effect discoveries, his own program could have been distinguished as "the expression of true relations between the facts it explains". Indeed, in an earlier remark he had admitted that during 1820 and 1821 he had rejected Biot's program "more due to the general order of the facts than by basing myself upon direct proofs".²³ Ampère did not rely upon novel predictions or 'use novelty' as criteria that could have supported his program prior to Faraday's discoveries.

Ampère's silence on this issue is not too surprising when we consider that he himself was making major theory revisions in response to new experimental phenomena. He responded to De La Rive's and Faraday's experimentation by revising his latest theory of magnetic circuitry. More fundamentally, during March 1822, Ampère produced a new variation of Faraday's rotary effects and discovered that this implied that one of the parameters in his force law should have the value $-\frac{1}{2}$ rather than the negligibly small value he had previously assigned it.²⁴ This was a major theory revision for Ampère, and it was partially responsible for an important set of experiments he performed in Geneva in September 1822. Although one of these experiments involved the detection of an effect brought about by electromagnetic induction, Ampère did not carry out the additional research required to achieve an accurate under-

standing of induction; Faraday only accomplished this nine years later. As I have explained in detail elsewhere,²⁵ one reason for this neglected opportunity was the far greater importance Ampère assigned to another experiment performed on the same day. This experiment confirmed a prediction Ampère had drawn from his revised force law. He had publically announced this prediction to the Académie on 24 June 1822, and he naturally provided some enthusiastic publicity when experiment confirmed his prediction. For example, the relevant passage from the text for Ampère's verbal report to the Académie concluded with the words: "Voilà donc un fait tout à fait inattendu, conclu d'avance de mes formules et vérifié ensuite par l'observation et de la manière la plus frappante."²⁶

As had been the case throughout his career, Ampère obviously still held the successful prediction of a new phenomenon to be an impressive piece of evidence. Nevertheless, although Ampère promptly placed his experimental discovery in a prominent position in an axiomatically-structured publication of 1822, he did not emphasize the fact that he had predicted it. Thus, as had been the case in 1821, during 1822 Ampère did not make any methodological proclamations that can be interpreted as evidence in favor of (GA1.3). Rather than highlighting novel phenomena as the decisive source of evidence, Ampère devoted extensive attention to a more general critique of Biot's static magnetic fluid program due to the problem posed by the rotary effects. The distinguishing factor Ampère cited most often was the fact that he could explain these effects on the basis of *moving* electric fluids. In conformity to (T1.1) and (T2.2), Ampère stressed his successful solution of problems that had stymied Biot; but there is no indication that Ampère considered "use novelty" to be the deciding issue.

7. 1823: SAVARY'S APPLICATION OF AMPÈRE'S PROGRAM TO MAGNETISM

Early in 1823 Félix Savary at last carried out the application of Ampère's force law to magnetic circuitries. Ampère had stressed the importance of these calculations almost two years earlier, and he immediately proclaimed Savary's accomplishment to mark "une sorte d'époque dans l'histoire de l'électricité dynamique".²⁷ Savary described his

primary concern to be an application of Ampère's force formula so as to study "l'analogie qui existe entre les aimans et des assemblages de courans électriques".²⁸ More precisely, in a subsequent abstract of his memoir, Savary summarized his derivation of Ampère's force law from a set of non-magnetic phenomena and then added that:²⁹

... M. Ampère's opinion on the constitution of magnets can acquire the type of proof demanded by the present state of physics only when, starting from this formula and by applying it to the circular currents he imagines within magnets, one will find by calculation the same results as those given by experiment, both with respect to the mutual action of two magnets and with respect to that between a magnet and a voltaic conductor.

After considerable labor, Savary was indeed able to derive, at least approximately, the following impressive set of previously established empirical laws: the Biot-Savart law for the effect of a current on a suspended magnet; Coulomb's law for the forces between magnetic poles; a hitherto unexplained effect produced by Ampère and Depretz in January 1821; laws pertaining to rotary effects established by Claude Pouillet; and a law of terrestrial magnetism studied by Biot and Bowditch. Savary's concluding remarks about all this were rather low-keyed.³⁰

The conclusion from these various results is that electrodynamic cylinders act, at least at distances not greatly exceeding their diameters, as do magnets for which the poles would be situated at the very extremities of these cylinders.

Savary made no attempt to draw a distinction between the effects that Ampère had relied upon to create his theory of magnetism and other effects not employed in this way. His discussion of his results shows no sign of any concern about the issue of 'use novelty'. On the other hand Savary was more sensitive to the criteria cited in (GA1.4) and (T1.1); in particular, he referred to the rotary effects and pointed out that only Ampère's program could explain why some of these phenomena could not be produced using magnets.³¹

Similarly, in his own comments on Savary's memoir, Ampère was more concerned about the superior explanatory scope of his program in comparison to Biot's than he was in drawing fine distinctions based upon 'use novelty'.³² It is true that in Ampère's private correspondence he took considerable pride in the fact that at a very early stage in his research he had foreseen the eventual quantitative confirmation of his magnetic theory. In 1820 he had published a set of 'conclusions' stipulating the basic assumptions of his reduction of magnetism to the action of electric currents. Writing to De La Rive in 1823, Ampère looked

back on this summary as "a kind of prevision of all that has developed since". Furthermore,³³

It is above all in comparing these conclusions with the results of all the experiments performed and all the calculations performed since that I congratulate myself in having written it; this is because I have always thought that there is all the more merit in having been the first to conceive truths which subsequently discovered facts confirm insofar as one had, at the time of this type of divination, as M. Biot calls it, less data with which to achieve this.

Ampère's pride is evident, but whatever he may have meant by "merit" in this passage, he did not make the clarifications that would have been required if he had wanted to alert De La Rive to the importance of the 'use novelty' criterion.

Similarly, when confronted by resistance to his program by Faraday, Ampère simply returned to the rhetoric he had used in 1821. For example, in a published version of an 18 April 1823 letter to Faraday, Ampère made the following claim about developments subsequent to the 1820 'Conclusions' memoir he had cited in his letter to De La Rive the previous month:³⁴

Subsequently, new phenomena, which I was not able to predict, have been discovered by various physicists; far from being found in opposition to my theory, they have all provided new proofs of it, or rather necessary consequences which it would have been able to predict in advance.

In spite of Ampère's claim to the contrary, in 1820 he obviously could not have predicted *all* subsequent discoveries. Some of these discoveries originally had been seriously anomalous and had required important revisions in both his force law and his magnetic theory. Secondly, Ampère made no attempt to stipulate which facts he had used to revise his theories; the criterion of 'use novelty' was no more operative during 1823 than it had been during the preceding period.

8. CONCLUSION

My conclusions can be summarized under four points. First, although in arguing for the acceptability of the guiding assumptions of his electrodynamics Ampère did maintain a robust respect for the successful prediction of new phenomena, he never pointed out in any clear way that such predictions should be recognized as special cases of 'use novelty'. He

surely would have done so if he had wanted 'use novelty' to be the primary mode of appraisal for his program.

Secondly, Ampère also relied upon other modes of appraisal such as ontological simplicity, restriction to central forces and overall empirical scope. He did not want his guiding assumptions to be judged 'largely' on the basis of 'use novelty'. Ampère's own promotion of his program thus presents evidence against (GA1.3).

Thirdly, however, John Worrall's version of the methodology of scientific research programs is not necessarily impugned by the foregoing conclusion. In addition to progressiveness with respect to the genuine empirical support provided by 'use novelty', Worrall also allows judgement of the 'heuristic strength' of a program to play an important role: in the present case it remains to be seen whether this notion can be rendered sufficiently precise to be tested.

Finally, the present essay obviously is not an exhaustive study of the modes of appraisal operative in early-19th-century electrodynamics. I have shown that Ampère did not argue in the way (GA1.3) would lead us to expect. This leaves open the possibility that other scientists, or even the majority of them, did assess rival programs largely according to the criterion in question. I shall close with some evidence that 'use novelty' was not the primary mode of appraisal among Ampère's colleagues; criticism was aimed primarily at Ampère's mathematical manipulations of current elements irrespective of his acknowledged explanatory success on the empirical level. For example, Ampère's close friend Maurice relayed an extensive critique from a correspondent who wished to remain anonymous. The objections included³⁵

... the large number of entirely gratuitous hypotheses, the abuse of the use of infinitely small magnitudes of which one can say whatever one wishes, and the mixing of various dynamical ideas for which the introduction is not sufficiently motivated and the influence is not precisely characterized.

These criticisms are motivated by a wide range of worries; no one criterion is cited as primary. I suspect that further research will confirm that this attitude was common among Ampère's critics and that the distinctions required to employ 'use novelty' as a decisive standard were simply too delicate to play a major role in the decisions of Ampère's contemporaries. Furthermore, it is clear that (GA4.5) is a far more accurate rendition of this period in the history of French electrodynamics than (GA4.4). Although most French physicists gradually conceded the superiority of Ampère's program over Biot's, 'elderly holdouts' such

as Poisson and Biot himself never abandoned research within the magnetic fluid program.

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NOTES

- ¹Lakatos [24], [25], and [26].
- ²Zahar [33] and Worrall [31] and [32].
- ³Gardner [19].
- ⁴Lakatos [24].
- ⁵Zahar [33] and Lakatos [26], p. 185.
- ⁶Zahar [33].
- ⁷Gardner [19].
- ⁸Worrall [31], p. 50.
- ⁹Blondel [17], Caneva [18], Hofmann [21] and [23] and Williams [29].
- ¹⁰Ampère tentatively adopted the molecular aspect of this assumption in January 1821 and it became an increasingly central assumption during the following few months; see Hofmann [22] and Williams [30].
- ¹¹Caneva [18].
- ¹²Ampère [1]. This and all other translations are my own.
- ¹³Ampère [2], p. 47.
- ¹⁴Ampère [3]. Ampère's remark conforms closely to the wording of (T2.4).
- ¹⁵Ampère [4], p. 549.
- ¹⁶Williams [29] and Hofmann [23].
- ¹⁷Hofmann [23]. In conformity to (T2.1), Ampère clearly was greatly encouraged about the status of his theory when he resolved this serious anomaly.
- ¹⁸Ampère [17] and Hofmann [21]. Ampère's strategy here offers a striking confirmation of (GA2.2) and (GA2.3); when his assumptions were confronted with empirical difficulties, he not only refused to change those assumptions but was quite willing "to leave the difficulties unresolved for years".
- ¹⁹See Ampère [7]. Also, throughout this period Ampère was using an incorrect value for one of the parameters in his force law. He did not correct this until April 1822.
- ²⁰Ampère [6], p. 319.
- ²¹Ampère [8], p. 128. Similar remarks appear in Ampère [9], pp. 306 and 308.
- ²²Ampère [11], p. 66.
- ²³Ampère [11], p. 64.
- ²⁴Hofmann, [21].
- ²⁵Hofmann, [22].
- ²⁶Ampère, [12], p. 331.
- ²⁷Ampère, [16], vol. 2, p. 624.
- ²⁸Savary [27], p. 1.

- ²⁹Savary [28], pp. 94–95.
³⁰Savary [28], p. 97.
³¹Savary [28], pp. 91–92.
³²Ampère [14], pp. 354–364.
³³Ampère [16], vol. 2, p. 627.
³⁴Ampère [15], p. 398.
³⁵Ampère [16], vol. 3, p. 924.

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